explanatory matter has been inserted occasionally, and the examples have been much improved by the inclusion of hints for their solution. In one or two places the clearness of the original is somewhat marred, as, *e.g.*, on p. 91, where two different notations are used simultaneously. It is not always obvious why formulæ have been banished from the text to separate lines, and *vice versa*; but this is a question of taste. The plates have been touched up here and there, and occasionally corrections have been made, *e.g.* ix., 3, 5; xiv., 10, 18.

It is surprising that the editor has retained the definition of curvature as measured by the *diameter of curvature*, whilst in at least one place he has used the first person in an interpolated remark.

The index and the classified list of curves are welcome additions. S. BRODETSKY.

An Account of the Crustacea of Norway. With short descriptions and figures of all the Species. By Prof. G. O. Sars. Vol. vi., "Copepoda, Cyclopoida." Parts 9–14. Pp. 105–225 + 54 plates. (Bergen: Published by the Bergen Museum, 1915–18.)

WITH the issue of the six parts noted above, Prof. Sars concludes the sixth volume of his great work on the Crustacea of Norway, the third of the series to be devoted to the rich and varied group of the Copepoda. This volume deals with the division Cyclopoida, which includes, for the most part, bottom-haunting species, many of them parasitic or semi-parasitic. Like the Harpacticoida treated of in the preceding volume, they are rarely found in the plankton, and must be sought for by special methods of collecting demanding much skill and patience. For this reason they have received far less attention than the relatively few species that are captured in bulk by the tow-net, and the proportion of novelties described in these volumes is very high. It is scarcely too much to say that Prof. Sars is giving us, for the first time, the means of forming a just impression of the Copepoda as a whole, both as regards their structural diversity and their habitats and distribution in northern seas. The species living in fresh water were previously better known, but here also Prof. Sars's accurate drawings will greatly facilitate the identification of species.

Apart from the faunistic value of the work, however, the iconography which it provides for many groups hitherto sadly in want of illustration will be of the greatest value to the morphologist and taxonomist. Among the numerous points of interest touched on in the parts under review, it may be noted that Prof. Sars revives Thorell's group of the Poecilostoma, although in a restricted sense and a subordinate position. In doing so he discards Claus's interpretation of the mouth-parts and reverts to that originally given by Thorell, according to which the mandibles, elsewhere so persistent, have entirely disappeared in the species composing this aberrant group.

W. T. C.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Perception of Sound.

I REGRET that I overlooked Prof. Bayliss's letter in NATURE of October 17, in which he made an appeal for my opinion. But, if I rightly understand, the question at issue seems to be mainly one of words. Can we properly speak of the propagation of sound through an incompressible fluid? I should answer, Yes. There may be periodic motion and periodic variation of pressure; the fact that there are no variations of *density* seems immaterial. Consider plane waves, corresponding with a pure tone, travelling through air. In every thin layer of air—and thin means thin relatively to the wave-length—there are periodic motion and periodic compression, approximately uniform throughout the layer. But the compression is not essential to the travelling of the sound. The substitution of an incompressible fluid of the same density for the gas within the layer would be no hindrance. Although there is no compression, there remain a periodic pressure and a periodic motion, and these suffice to carry on the sound.

The case is even simpler if we are prepared to contemplate an incompressible fluid without mass, for then the layer need not be thin. The interposition of such a layer has absolutely no effect, the motion and pressure at the further side being the same as if the thickness of the layer were reduced to zero. To all intents and purposes the sound is propagated through the layer, though perhaps exception might be taken to the use of the word *propagation*.

As regards the ear, we have to consider the behaviour of water. From some points of view the difference between air and water is much more one of density than of compressibility. The velocities of propagation are only as 4 or 5 to 1, while the densities are as 800 to 1. Within the cavities of the ear, which are small in comparison with the wave-lengths of musical sounds, the water may certainly be treated as incompressible; but the fact does not seem to be of fundamental importance in theories of audition.

RAYLEIGH.

Terling Place, Witham.

The Common Cause of Pure and Applied Science.

A GOOD deal of anxiety has been expressed recently in various quarters lest the great interest now being evinced in applied science may perpetuate, or even aggravate, the national neglect of pure science. As I do not share this anxiety, but, on the contrary, am strongly convinced that exactly the opposite effect will ensue; and as there seems to be some danger in the attitude that is being assumed by certain of my friends, I should like, with great respect, to ask for attention to the considerations which have led me to these opinions.

For many years past there has been in this country what the late Lord Armstrong once well called "a vague cry for technical education," a dim feeling on the part of the industrial world, collectively speaking (there have, of course, been brilliant individual exceptions), that there was some business-end of science that was worth getting hold of, and that should be got on easy terms of talent, time, and money. We know what it has failed to produce in institutions, in individuals, and in industrial efficiency; and we knew

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it would. My working life has been passed in a great industrial region where this faint-hearted belief in the utility of science has been the one real obstacle to the progress of good science of every kind. At Leeds I have occupied myself greatly with the promotion of applied science, as in duty bound. But it has also been in the sure and certain hope that applied science, worthy of the name and really worthy of acceptance by industry, was indissolubly linked in bonds of mutual benefit to the purest and highest science that was ever dreamed of even by my chemical brethren, whose unworldly "stinks" profane the cloisters of more sequestered seats of learning.

It has been a hard fight, and though it would be unjust to say there have been no gains, I long since came to the conclusion that nothing short of a national cataclysm was likely to bring about anything approaching the change of heart that was so desirable and so necessary.

The cataclysm of war has, in fact, done this great thing for science. There is indisputable evidence of it, and I believe that at last British industry is generally, not exceptionally, on its way to use science well. That being so, I ask: Is there any possible escape for British industry and the British public from promoting pure science, and promoting it handsomely? I do not see it. Of course, they will not begin by endowing professorships in radio-activity or relativity, nor yet, perhaps, in that very pure chemistry which is the dearest thing to me; but they will be obliged to do it, and to do it before long. In the first instance, they will ask for what they now know they want: first-rate men who can apply science to the practical problems of industry. Already to a large extent they know that such men must have in them the root of the matter in the form of real scientific knowledge and skill, and it will follow as the day the night (if you so regard it) that science, pure and simple, must also be the object of their self-interested or patriotic solicitude.

I, for one, shall be glad to have it on those terms. For what, let us frankly say, are the alternatives for pure science? One I have just tried to set forth; the other, it seems to me, is a direct appeal for pure science, either because it is pure or because it is useful. If you extol it because it is pure, it is a worthy effort that I should honour with all my heart on one condition, and that is that you should avoid the incalculable mischief of trying to make out that there is in essence any distinction between pure and applied science, or that you should give just cause for the belief that there exists a brotherhood in science who set themselves up as the elect and disdain the implications of science in the practical arts that serve and preserve mankind.

If you extol pure science simply because it is useful—which by hypothesis you do not want to do—you embark on the task, long since essayed and long sustained, of teaching people by exhortation what at last they are in the way of finding out surely for themselves. To do that runs counter to all the precepts I have drawn from my experience of teaching.

I know very well what it is to be a prophet of pure science, even if only a minor or a minimus one, crying in the wilderness, and believe I can enter somewhat into the feelings of the major and maximus ones who are anxious and impatient under the present aspect of affairs. But they may be asked earnestly to consider the other point of view also, and to bethink themselves whether, after all, a great deal of the Philistinism of our people is not due to the detachment of locality, of interest, and of intercourse that in the past has been justly chargeable to the world of learning.

The British Association for the Advancement of Science was founded for the purpose of bringing a knowledge of science, its glories and its uses, among the people. It has done a great work, a much greater work than is known to those who will not sacrifice a week of the Alps or the oceans to do their bit and to experience the stimulus and profit derivable from the meetings—chiefly, it must be admitted, outside the section-rooms. The British Association needs revitalising, and I believe it can be revitalised. If our men of science would rally to it, it might do much that seems either to be neglected or to be falling into the hands of new organisations, the number of which alone, to say nothing of their particular distinctions or their subscriptions, is becoming quite bewildering.

It is, of course, the British way to have a multiplicity of disconnected organisations doing, or trying to do, much the same thing. We have won the war (it is true some others "also ran"), and Britain is justified in her institutions. To that no one subscribes more heartily than I, but we made some mistakes; and though organisation in the German way may be the mental path to inhumanity if followed far, I think we might profit by using a little more co-operation as we go our several ways.

Chemistry, it has been said, is a French science. Be that as it may, the immortal Lavoisier, who did more than anyone to revolutionise chemistry, began to investigate combustion because he was interested in lighting the streets of Paris. So at least says M. Le Chatelier, who is, I think, a chemist *assez pur*. According to my reading of history, so much pure science has arisen, not from the heavens above, but from the earth earthy beneath, that I will never, if I can help it, be penned off by any principality or power from the fraternity of applied science. Besides that, I owe them personally more than can ever be acknowledged for heading me off certain great dangers that threaten the academic life, and for helping me in countless ways with the promotion of pure science. We may rejoice without reserve in their temporary monopoly of popular favour.

Arthur Smithells.

The Theory of Hormones Applied to Plants.

No one would have read Prof. Bayliss's review (Nature of December 12, p. 285) of Dr. Jacques Loeb's experiments on the "chemical correlationship" in plant growth with greater interest than John Hunter, for he had carried out many experiments on growing beans to elucidate the phenomena which are now explained on the theory of hormones. Hunter was familiar with phenomena of a similar kind in animals, and his experiments on plants were made primarily to elucidate that mysterious mechanism which went in Hunter's time under the name of "sympathy." An account of Hunter's experiments, carried out between 1772 and 1790, will be found in "Essays and Observations by John Hunter," edited by Sir Richard Owen, and published in 1861 (vol. i., p. 367). These observations were saved from destruction by William Clift.

Arthur Keith.

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RESEARCH ASSOCIATIONS AND OTHERS.

RESEARCH is the cry in every direction, but the public still needs instruction as to what it means and the conditions requisite for progress. Discovery of new principles on which advance can be made in the fundamental knowledge of Nature will probably be accomplished in the future, as in the past, through the genius of the few gifted men, but the dissemination of the right kind of knowledge and the creation of a widely

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